Trophic uptake and entry of aquatic contaminants to the Estuarine food web: the role of particulate organic carbon

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Public Comments

No public comments were received for this proposal.

Collaboration Panel Review

Proposal Title

#0216: Trophic uptake and entry of aquatic contaminants to the Estuarine food web: the role of particulate organic carbon

Final Panel Rating
adequate

Collaboration Panel (Primary) Review

Collaboration:

Will the results of the collaborative effort be greater than the sum of its parts? Is it clear why the subprojects are part of a larger collaborative proposal rather than several independent smaller ones?

This $1 million collaboration investigates toxicity to rotifers and effects on delta smelt growth, histopathology, and endocrine disruption from waters in Suisun marsh. This toxicity is then partitioned between DOM and POM. No aspect of the project works alone independently.

Interdependence And Integration:

Does the proposal have an example that clearly articulates the conceptual model of each subproject and how they link together as a whole? Are the boundaries of the study plans focused and cohesive, yet well delineated? Is there a plan for potential differences in the stages of subproject completion times? Are there clear plans for analyses and interpretations which seek to identify and quantify relationships among the data collected in various subprojects rather than separate analyses for each subproject?

The hypotheses are clearly stated, and there is a good figure demonstrating the rationale of the work plan among tasks. What I couldn’t determine from the proposal is how the results will be integrated at the end of the project.

Project Management:

Is it clear who will be performing management tasks and administration of the project? Are there resources set aside for project management and time given for investigators to collaborate? Is there a process for making decisions during the course of the project? Are there acknowledgments of potential barriers to collaboration and explanations of how team members will overcome barriers particular to their institutions?

Project management is not discussed, but is indicated to be available on line (no html is given and I didn't check), but the UCD team has a proven collaboration with LLNL.

Team Composition:

Does the lead principal investigator have successful management history and experience leading collaborative teams? Is it clear that all key personnel are committed to making significant contributions to the project? Do team members have complementary skills?

The PI has experience managing big projects. There's clear commitment of the team members to the project. It wasn't clear to me how the team picked B. plicatilis. As a result, I wondered if the team needed some more expertise in zooplankton ecological energetics to be effective.

Communication Of Results:

Is there a clear plan for comprehensive and cohesive reporting of project progress to the CALFED community?

The team proposes 4–6 papers, IEP workshops, and coordination with RMP monitoring program summaries. No integrative paper is specifically proposed.

Additional Comments:

Collaboration Panel (Discussion) Review

The primary reviewer judged the proposal as Adequate. After a panel discussion, the reviewer adjusted upward his score of project management and felt that the work distribution is more well defined as he had previously thought. Secondary reviewer agreed with most comments of the Primary reviewer, and noted that there was no well−devised plan for final study integration; her final rating is Adequate.

Technical Synthesis Panel Review

Proposal Title

#0216: Trophic uptake and entry of aquatic contaminants to the Estuarine food web: the role of particulate organic carbon

Final Panel Rating
adequate

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

While the proposed project appears to be important and the qualifications of the PI appear to be good, the overall proposal is very ambitious. The budget is also very large. It is good to try to link ecological and toxicity aspects of the study, but I do not feel that the sampling and coordination of ecosystem and toxicity controls are well enough designed.

Additional Comments:

While the proposed project appears to be important and the qualifications of the PI appear to be good, the overall proposal is very ambitious. The budget is also very large. It is good to try to link ecological and toxicity aspects of the study, but I do not feel that the sampling and coordination of ecosystem and toxicity controls are well enough designed.

Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

The project addresses a very important set of concerns in the CBDA-solution area. The proposal is very ambitious. A great

deal of research has been conducted on the complex nature of organic matter with dissolved particulate interactions and abiotic−biotic dynamics. To examine anthropogenic influences is a very difficult undertaking. The panel was not convinced that the research team was adequate to the tasks outlined in the proposal in that they lacked background in certain areas of this multi−faceted proposal. The proposal would have benefited from a greater discussion of the “boundaries” of probable contributions of different sources of contaminants. There is substantial research on organic carbon dynamics in this system and some ballpark estimates of the likely effect of different OC constituents would have been helpful.

Technical Review #1

proposal title: Trophic uptake and entry of aquatic contaminants to the Estuarine food web: the role of particulate organic carbon

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The overall goals of this proposal are aligned well with the goals of the RFP. The proposal seeks to understand the spatial and temporal variation in contaminant exposure in the SSJRs, and to determine the degree to which contaminants sorb to POM. POM−sorbed contaminants may be especially problematic to aquatic food webs because zooplankton and larval fishes may consume these directly. The hypotheses are clearly stated and generally consistent. Hypothesis 6 will only be addressible in an indirect way. The proposed work will be able to show that the contaminants increase (or not) mortality rates of juvenile smelt, but the work will not be able to directly assess whether these changes in mortality are enough to account for changes in the population dynamics of the Delta smelt. To do this, the PIs would have to develop population models that account for density−dependence in growth and survival to explore the ultimate consequences of contaminants for changes in the populations of Delta smelt.**
Rating	**very good**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**This research is strongly justified. Contaminants are one of several existing hypotheses to explain the observed changes in the San Francisco Estuary food webs. This research will make substantial advances in determining whether or not this is a reasonable hypothesis. The conceptual model is straightforward and supports the proposed research.**
Rating	**excellent**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**The approach used in this study appears to be appropriate for meeting the goals of the research. The experiments are generally well designed (although see comment above concerning linking contaminant sensitivity to population dynamics). I would have preferred to see the zooplankton toxicity assays to include copepods as well as rotifers given that copepods used to be the dominant prey item for Delta smelt. However, culturing copepods is substantially more complicated than for rotifers and this may not be practical because it would require that the assays be run substantially longer to account for the longer life-cycle of the copepods.** **A substantial improvement in this project would have been to include population modeling of both the smelt and the rotifers to see how much of a population effect the comtaminant exposures would have. Nevertheless, the data and knowledge produced in this proposal will be readily available for such modeling efforts pursued by other scientists in the future.**
Rating	**very good**

Technical Review #1

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**The approach is well documented and the investigators appear to be well qualified to accomplish their goals. The team involves a set of investigators with highly complementary skills and experience. Apparently all of the necessary contacts for obtaining test organisms have been made. The investigators appear to have access to all the necessary instruments needed for this study.**
Rating	**excellent**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**It would be nice to see more than 2 samples per year to account for the full possible range of seasonal variation in contaminant concentrations and POM associations. However, it might have been cost−prohibitive to accomplish this. It is not clear that the 'winter storm' period and the 'summer low flow' period are the most appropriate times to sample the estuary. The investigators should consider sampling at the time of year that overlaps the most with Delta smelt larval abundance (it is possible that the existing sampling scheme already does this).**
Rating	**good**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**The products produced from this project will have substantial value. The data will provide invaluable information needed to assess the contaminant hypothesis as a driver of observed changes in the San Francisco estuary. The investigators appear to be motivated and organized to make their data available to other researchers.**
Rating	**very good**

Additional Comments

Comments	**none**

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**The two senior investigators have strongly complementary skills needed to accomplish their desired research. They have been sucessful at publishing their previous research and have extensive experience in the proposed research areas. They have organized a team of technical staff to support their research tasks. As mentioned above, all of the necessary infrastructure to finish this research appears to be in place.**
Rating	**excellent**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**It is somewhat difficult to assess this budget. Contaminant research is not cheap so this probably justifies the considerable budget. It would have been easier to assess the budget had the personnel time**

	been summarized on a monthly basis for the overall project rather than on an hourly basis by project task. The costs of the subcontractors are big and there is no way to assess how reasonable they are. **Overall, I do not have a strong feeling for how apprpriate the budget it. Given the experience and success of the investigators in the past, I have to assume that it is reasonable.**
Rating	**very good**

Overall

Provide a brief explanation of your summary rating.

Comments	**Overall, this is a solid proposal that is aligned well with the goals of the grantor. The toxicity testing and chemical characterization of possible contaminants is an important component of developing an understanding of the roles of contaminants versus other agents of disturbance (e.g., climate, exotic species, etc.) in the San Francisco estuary food webs.** **My main hesitation with this project is the tendency for toxicity studies to be poorly integrated in an ecosystem context. It is still unclear how to link toxicity tests in beakers to ecological dynamics in the field. Modeling is one step towards achieving this but it not listed as a part of this proposal. The data that will be produced by this proposal will help future modeling efforts but will be difficult to place into an ecological context before that happens. Nevertheless, this research is well designed and will improve our understanding of the anthropogenic stresses to the San Francisco estuary.**
Rating	**very good**

Technical Review #2

proposal title: Trophic uptake and entry of aquatic contaminants to the Estuarine food web: the role of particulate organic carbon

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The goals for this proposal are wide in scope covering the potential impacts of the toxicity of San Francisco Estuary water on fish and zooplankton as well as the role of organic matter in altering the contaminant bioavailability within the estuary. Thus, this proposal has broad implications for both the toxicity and the ecology of the San Francisco Estuary as well as estuarine habitats in general. The focal species, the Delta smelt is a Federally-listed Threatened Species, thus this study has direct implications for the near and long-term management of this fish species.** **The one area that is not consistent within the goals, hypotheses and experimental design is the hypothesis (H2) that "contaminant-associated toxicity to zooplankton is severe enough to limit its availability as a food source to Delta smelt." This hypothesis is not experimentally tested within the projects outlined within this proposal, thus should not be included. The idea that there was a trophic link between zooplankton decline and smelt decline was strongly supported within the initial justification of the proposal, but was not experimentally addressed. While this over-sight does not detract from the merit of the remainder of the study, it does limit the scope of the conclusions that can be drawn from the study. As it stands, the experiments will provide information that**

	may lead to a better understanding of the possible effects of contaminants on the trophic interactions within the estuary.
Rating	**very good**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**This study proposes to establish the ecological relevance, as well as the potential toxicity of environmental contaminants. This is a unique approach and should be successful in improving our understanding of how measured levels of environmental contamination relate to the survival of aquatic organisms. The authors provide a sound conceptual model demonstrating the need, as well as the feasibility of the experiments that they propose to undertake. The level of current knowledge is sufficient to justify full−scale implementation of this project.**
Rating	**excellent**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**The approach used to address the goals of this project are one of the strong elements of this proposal. The three tasks are certainly feasible and laid out in a sensible and productive manner. The management of the project (Task 1) maximizes the skills of the scientists involved with the project and all**

	researchers appear to have an excellent approach to data management and quality assurance– necessary details for a study with broad resource management implications. The second task, with the exception of two minor caveats (see next section) is well designed to assess the toxicity of the ambient estuarine water for both zooplankton and Delta smelt. The second task logically leads into the third, and largest task where the authors propose to examine the effects of the organic matter fractions on the toxicity of the estuarine waters. It is the third task that has a strong likelihood of providing exciting and novel information concerning the role of organic matter in controlling the toxicity of estuarine water. The information generated from this research should influence future management of the San Francisco estuary and its tributaries. The knowledge gained will likely improve the capabilities of scientists to judge when and where the water contaminants will have toxic effects within the estuary.
Rating	excellent

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	While this project is ambitious and broad in scope the authors are well suited for accomplishing the three major tasks involved in the project. Dr. Teh is qualified to be a lead investigator on this large–scale project with experience directly related to the proposed research and a track record for timely publication of results. The additional scientists on the project, Drs. Hall, Love, Deng and Hamilton are a sufficiently qualified to manage and successfully carry out their individual portions of the project. There are two issues with the feasibility of this study. First, the sampling of the estuarine water is

	proposed to occur after a winter storm and subsequently during the late summer. The authors do not provide any information about how they will assess the appropriate timing of these collections. It is critical for the success of the project that they occur during time periods when contaminant levels are high, but there is no indication what the a priori conditions are for initiating sample collection. For example, what constitutes a winter storm or what are the low water flow levels necessary to concentrate contimants. Furthermore, the authors offer no contingency plans if weather events do not occur at the appropriate time or at a sufficient intensity during the first year of the study. Second, the authors proposed research is dependent upon some level of toxicity being found within the water samples. As before, the authors do not offer a contingency plan if they do not find the expected levels of contamination or toxicity. Given that the project is dependent upon in situ sampling, while perhaps only a small risk, there is the possibility that the water samples will have low contaminant loads. While these are details that do not significantly detract from the over−all scientific quality of the proposal, there is a risk that the major goals of this 3−year proposal may not be achieved.
Rating	**very good**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**This study is well designed with appropriate controls and a strong approach to appropriately utilize and disseminate the data. The one question that is not fully addressed is how the authors will be able to take their data set and make broad−trophic level ecologically based conclusion and/or regulatory decisions.**

Rating	**excellent**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**The outcomes of this proposed research are highly likely to contribute to the broader understanding of the effects of contaminants within the California Bay delta on smelt as well as other fish species. The authors have a clear plan for disseminating the information and including regulatory agencies in the process.**
Rating	**excellent**

Additional Comments

Comments	**The inclusion of assays for the effects of endocrine disrupting contaminants is quite important as this is certainly of growing concern. However, it is important that the authors not limit themselves, at least conceptually, to the notion that endocrine disruption occurs via the estrogen receptors, or even through the hypothalamo−pituitary−gonadal axis. Endocrine disruption can occur through many different targets (e.g. androgen receptors, stress axis and thyroid glands).**

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**The authors have a strong track record in conducting similar research projects, and particularly large−scale projects such as the one proposed. The**

	team that is assembled to carryout this research project have broad areas of expertise and are affiliated with laboratories that have the necessary infrastructure to successfully complete the project.
Rating	excellent

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	The budget, while large, is appropriate for the amount and scope of work involved with this study. The proposed research requires scientists with expertise in water chemistry, toxicology and histopathalgy. Thus, the number of researchers (6) involved with this project are appropriate.
Rating	excellent

Overall

Provide a brief explanation of your summary rating.

Comments	This proposal has the overall goal of examining the role that organic matter plays in contaminant bioavalability to Delta smelt and their zooplankton prey. The research scope includes testing the impact of organic matter fractions from the ambient San Francisco bay waters on the contaminant bioavailability and attempting to relate the information to the broader ecological implications of trophic level impacts. Thus, the proposal is broad in concept and is attempting to address important questions regarding the toxicology, the environment and Delta smelt survival. The authors provide a logical and reasonable approach to address these important questions. With only a few minor exceptions the proposal

	is sound with a strong hypothesis driven set of questions that are highly feasible. Thus, the likelihood for success is great, and the questions being addressed are both timely and novel.
Rating	**very good**

Technical Review #3

proposal title: Trophic uptake and entry of aquatic contaminants to the Estuarine food web: the role of particulate organic carbon

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The goals of the proposal are logically explained and consistent. The connection between organic matter, contaminants, and the food web is a poorly understood and recognized problem in environmental biogeochemistry and the idea for the proposal is well founded. How important this connection is for the abundance of delta smelt is not clear since they seem to be effected mainly by habitat loss. Goals 2−4 are very ambitious and will be very difficult to address technically. This is because the majority of natural organic matter can not be characterized on a molecular level making an association of organic matter composition and their role in contaminant transport almost impossible.**
Rating	**very good**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**The decline in zooplankton and Delta smelt can not conclusively be linked to the appearance of contaminants in the Delta; it needs to be clear what the relative contributions of habitat loss and**

	contamination are. The proposal states a clear conceptual model but the underlying basis for the project are not explained in enough detail to link organic matter composition to contaminant bioavailability and effects on organisms. A pilot study might be more appropriate than a full-scale project.
Rating	**good**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**For most of the listed tasks the approach and procedures are well explained and reproducible. However, for task 3 the proposal never explains how DOM fractions are separated from water sample. The DOM fractionation scheme is also very poorly explained and references to other work might not be appropriate since the study is dealing with a mix of natural DOM (mostly unknown) and not with fresh plant derived material as in the Robinson reference. The characterization of organic matter using HPLC-ESI-MS is not explained at all and references are missing. Again, most natural organic matter is uncharacterized and characterization by HPLC-ESI-MS has not been shown. The analysis of water samples for contaminants and toxicity testing appear solid and will add new information to evaluate the role of those contaminants for the reproducibility and abundance of zooplankton and Delta smelt.**
Rating	**fair**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**In agreement with what was said before many aspects of the proposal seem feasible and the necessary infrastructure is also in place. The senior personnel have much experience and can be expected to successfully accomplish most of the planned research. The weak point, again, is the chemical characterization of natural organic matter and the linkage of such to the toxicity and bioavailability of contaminants. It is also questionable if the person responsible for natural organic matter characterization has the necessary experience to tackle such a challenging task on his own. Dr. Love is a postdoctoral fellow with limited documented experience in HPLC−ESI−MS, particularly on natural organic matter and might leave the project before its completion.**
Rating	**good**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**The study is mainly observational/experimental and not a classical monitoring program**
Rating	**not applicable**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**Expected products of this study are primarily of scientific and public value. The toxicity testing will indicate if and how contaminants affect the ecology of zooplankton and Delta smelt. The generated data will contribute to existing large data sets at UCD as well as CALFED and will also be made available to the public through the internet. The outcome of the project should also initiate further research in the complex area of organic matter–contaminant interaction and its role for the food web.**
Rating	**good**

Additional Comments

Comments	**The proposed study has several very attractive aspects that are well presented and likely lead to important new information for the Delta area. Other aspects, in particular the fractionation and characterization of natural organic matter are very speculative and not well founded here. The project might be stronger by focusing on the strong aspects and removing the fractionation and chemical characterization part. Alternatively, the project could be changed into a pilot study to gather initial data on the more "risky" portion of the project.**

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**It seems that all the necessary infrastructure is in place to perform the proposed work. The senior personnel is apparently qualified for their portion of the project, however, the chemical characterization of natural organic matter appears too challenging for only one person with relatively little experience in**

	the specific subject.
Rating	**good**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**The proposal represents an interesting but not exceptional study with a number of uncertainties. By reducing the number of tasks or changing to a pilot study the budget could be reduced. In comparison to standard 3 year projects funded by NSF, 1,000,000 seem expensive.**
Rating	**fair**

Overall

Provide a brief explanation of your summary rating.

Comments	**The proposed project has many different components with several very challenging aspects. Most of the proposed work seems well founded and feasible. The general question behind the proposal is important but might also be effected by other factors, as indicated by the authors. I would consider the proposal question to be of moderate priority with few aspects that seem too ambitious and do not have enough background information.**
Rating	**good**